DOCUMENT RESUME

ED 461 710 UD 034 726

AUTHOR	Angrist, Joshua D.; Bettinger, Eric; Bloom, Erik; King, Elizabeth; Kremer, Michael
TITLE	Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment. NBER Working Paper Series.
INSTITUTION	National Bureau of Economic Research, Cambridge, MA.
REPORT NO	NBER-WP-8343
PUB DATE	2001-06-00
NOTE	53p.
AVAILABLE FROM	Working Papers, NBER, 1050 Massachusetts Avenue, Cambridge, MA 02138-5398 ($10). Tel: 617-868-3900; Fax: 617-868-2742. For full text: http://www.nber.org.
PUB TYPE	Reports - Research (143)
EDRS PRICE	MF01/PC03 Plus Postage.
DESCRIPTORS	*Academic Achievement; *Educational Attainment; *Educational Vouchers; Expenditure per Student; Foreign Countries; Outcomes of Education; Poverty; *Private Schools; School Choice; Secondary Education
IDENTIFIERS	*Colombia

ABSTRACT

This paper examines the impact of Colombia's Programa de Ampliacion de Cobertura de la Educacion Secundaria (PACES), which provided over 125,000 poor students with private secondary school vouchers, many of which were awarded by lottery. Researchers surveyed lottery winners and losers to compare educational and other outcomes. Results showed no significant enrollment differences between winners and losers 3 years after application, with most students in both groups still in school. Lottery winners were 15 percentage points more likely to attend private than public schools. They had completed an additional .1 years of school and were 10 percentage points more likely to have completed 8th grade, primarily because they repeated fewer grades. Winners scored .2 standard deviations higher on standardized tests. PACES did not affect dropout rates significantly. Lottery winners were less likely to be married and cohabiting and worked about 1.2 fewer hours per week than losers. They increased their educational expenditure by about 70 percent of the value of the voucher. The voucher program increased annual government educational expenditure by $24 per winner, but costs were probably much less than increases in the winners' earnings due to greater educational attainment. (Contains 10 tables and 43 references.) (SM)

Reproductions supplied by EDRS are the best that can be made
from the original document.

ED 461 710

WORKING PAPER SERIES

VOUCHERS FOR PRIVATE SCHOOLING IN COLOMBIA: EVIDENCE FROM A RANDOMIZED NATURAL EXPERIMENT

Joshua D. Angrist
Eric Bettinger
Erik Bloom
Elizabeth King
Michael Kremer

WORKING PAPER 8343

NATIONAL BUREAU OF ECONOMIC RESEARCH, INC.

PERMISSION TO REPRODUCE AND DISSEMINATE THIS MATERIAL HAS BEEN GRANTED BY

J. Angrist

TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

1

U.S. DEPARTMENT OF EDUCATION
Office of Educational Research and Improvement
EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

☑ This document has been reproduced as received from the person or organization originating it.

☐ Minor changes have been made to improve reproduction quality.

• Points of view or opinions stated in this document do not necessarily represent official OERI position or policy.

BEST COPY AVAILABLE

UD034726

You can download this and other papers at the NBER Web site:

www.nber.org

Free searchable abstracts are also available at the site.

Please see the back pages of this paper for a listing of current working paper titles and subscription ordering information.

The National Bureau of Economic Research is a private, non-profit, non-partisan organization engaged in quantitative analysis of the American economy.

This paper has not undergone the review accorded official NBER publications; in particular, it has not been submitted for approval by the Board of directors. It is intended to make results of NBER research available to other economists in preliminary form to encourage discussion and suggestions for revision before final publication.

NBER WORKING PAPER SERIES

VOUCHERS FOR PRIVATE SCHOOLING IN COLOMBIA: EVIDENCE FROM A RANDOMIZED NATURAL EXPERIMENT

Joshua D. Angrist
Eric Bettinger
Erik Bloom
Elizabeth King
Michael Kremer

Working Paper 8343
http://www.nber.org/papers/w8343

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2001

Special thanks go to the survey and field team in Bogota: Claudia Gonzalez, Marcela Monsalvo, and Ana Gomez; in Boston, we had the help of Helen Lee, Brian Pasquinelli, and especially Cristina Estrada. We are also grateful to Jorge Estrada for help interpreting Colombian ID numbers, and to Jose Uribe for arranging for use of a testing site. Finally, thanks go to the World Bank and the National Institutes of Health for funding, and to Alberto Abadie, Jere Behrman, David Levine, Adriana Kugler, Lant Pritchett, Petia Topalova, and seminar participants at Banco de la Republica, Berkeley, Harvard, MIT, the NBER, Northwestern, and Princeton for comments. The views expressed herein are those of the authors and not necessarily those of the National Bureau of Economic Research.

© 2001 by Joshua D. Angrist, Eric Bettinger, Erik Bloom, Elizabeth King, and Michael Kremer. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Vouchers for Private Schooling in Colombia:
Evidence from a Randomized Natural Experiment
Joshua D. Angrist, Eric Bettinger, Erik Bloom, Elizabeth King, and Michael Kremer
NBER Working Paper No. 8343
June 2001
JEL No. I2, H4, O5

ABSTRACT

Colombia's PACES program provided over 125,000 pupils from poor neighborhoods with vouchers that covered approximately half the cost of private secondary school. Since many vouchers were allocated by lottery, we use differences in outcomes between lottery winners and losers to assess program effects. Three years into the program, lottery winners were 15 percentage points more likely to have attended private school, had completed .1 more years of schooling, and were about 10 percentage points more likely to have finished 8 th grade, primarily because they were less likely to repeat grades. The program did not significantly affect dropout rates. Lottery winners scored .2 standard deviations higher on standardized tests. There is some evidence that winners worked less than losers and were less likely to marry or cohabit as teenagers. On average, lottery winners increased their educational expenditure by about 70% of the value of the voucher. Since winners also worked less, they devoted more total resources to education. Compared to an equivalent expansion of the public education system, the voucher program increased annual government educational expenditure by about $24 per winner. But the costs to the government and to participants were probably much less than the increase in winners' earnings due to greater educational attainment.

Joshua Angrist
MIT Department of Economics
50 Memorial Drive
Cambridge, MA 02142
and NBER

Eric Bettinger
Department of Economics
Weatherhead School
Case Western Reserve University
Cleveland, Ohio 44074

Erik Bloom
Center for International Development
Research Triangle Institute
3040 Cornwallis Rd
RTP, NC 27709-2194

Elizabeth King
Development Research Group
The World Bank
1818 H Street NW
Washington DC 20433

Michael Kremer
Department of Economics
Littauer Center 207
Cambridge, MA 02138
and NBER

The question of who should provide education is nowhere more pressing than in developing countries. While the academic controversy over school providers and school vouchers has raged most intensely in the US, private schools account for only about 11% of US enrollment (USDOE, 1998). Moreover, over half of American parents report that they are very satisfied with the public schools their children attend. In the developing world, in contrast, private enrollment as a proportion of total enrollment is 2-3 times higher than that in industrialized nations (James, 1993). Problems with public schools are usually more severe in low-income countries, since the quality and integrity of public-sector service-delivery is highly correlated with income levels (Rauch and Evans, 2000). In Indian schools, for example, a recent study found that one-third of headmasters were absent at the time of the researchers' visit (PROBE Team, 1999), while in Kenya, Glewwe, Kremer, and Moulin (2000) found that teachers were absent 28% of the time. The view that private schools function better than public schools in the developing world has prompted calls for governments in poor countries to experiment with demand-side financing programs such as vouchers (e.g, Psacharopolous, Tan, and Jimenez, 1986).

This paper presents evidence on the impact of one of the largest school voucher programs to date, the Programa de Ampliación de Cobertura de la Educación Secundaria (PACES), a Colombian program which provided over 125,000 pupils with vouchers covering somewhat more than half the cost of private secondary school. Vouchers could be renewed as long as students maintained satisfactory academic performance. Since many vouchers were awarded by lottery, we use a quasi-experimental research design comparing educational and other outcomes of lottery winners and losers. Subject to a variety of caveats, the resulting estimates provide evidence on program effects similar to those arising from a randomized trial. As far as we know, ours is the first study of a private school voucher program in a developing country to take advantage of randomized treatment.[1]

---

[1]US studies in this mold include Green, Peterson, and Du (1996) and Rouse (1998), who evaluated a voucher lottery in Milwaukee. Rouse's estimates, which control for attrition, show modest increases in math scores among voucher recipients. Other US studies include Howell *et al* (2000), Myers *et al* (2000) and Bettinger (2001a), who evaluate various private scholarship programs. Also related is McEwan and Carnoy (1999), who report differences-in-differences estimates of spillover effects from a large-scale voucher program in Chile. Bellow and King (1993) assess a smaller program in Bangladesh. The literature on public/private comparisons in the US is extensive. Two recent studies are Evans and Schwab (1995) and Neal (1997). Cox and Jimenez (1991) compare public and private

Our research strategy was to survey lottery winners and losers from three applicant cohorts. The results from this survey show no significant enrollment differences between lottery losers and winners three years after application, with most pupils in both the winner and loser groups still in school. But lottery winners were 15 percentage points more likely to attend private schools rather than public schools. Moreover, lottery winners had completed an additional .1 years of school and were about 10 percentage points more likely than losers to have completed 8th grade, primarily because they repeated fewer grades. Although high rates of grade repetition are a widely recognized problem in Latin America (see, e.g., Jacoby, 1994; and Psacharopoulos and Velez, 1993), reduced repetition need not indicate greater learning. We therefore administered achievement tests to a subset of the pupils surveyed. The test results suggest that, on average, lottery winners scored about .2 standard deviations higher than losers, a large but only marginally significant difference. The effect on girls is larger and more precisely estimated than the effect on boys.

In addition to increased educational attainment and academic achievement, there is also some evidence that the voucher program affected non-educational outcomes. In particular, lottery winners were less likely to be married or cohabiting and worked about 1.2 fewer hours per week (again, mostly a difference for girls). Both of these results suggest an increased focus on schooling among winners.

While comparisons between winners and losers provide a simple strategy for assessing program impact, our survey indicates that only about 90% of lottery winners had ever used the voucher or any other type of scholarship, while 24% of losers received scholarships from other sources. It therefore seems reasonable to think of lottery win/loss status as an instrument for scholarship receipt in a two-stage least squares (2SLS) set-up. There is a strong first-stage here, though the relationship between voucher status and scholarship use is not deterministic. Instrumenting for scholarship use with lottery win/loss status suggests that scholarship use generated effects on grade completion and test scores that are roughly 50% larger than

---

schools in Colombia and Tanzania, and Jimenez, Lockheed, and Paqueo (1991) summarize comparisons in five countries. See also the Patrinos and Ariasingham (1997) survey of demand-side financing in poor countries. Glewwe, Kremer, and Moulin (2000) and Behrman, Sengupta, and Todd (2000) use randomization to examine other educational interventions in developing countries.

the reduced form effect of winning the lottery.

The last part of the paper presents a fiscal and cost-benefit analysis of the voucher program. Most lottery winners would have attended private school anyway, at least for a few years, and therefore reduced their educational expenditure in response to the program. On the other hand, voucher winners who were induced to switch from public to private schools greatly increased their educational expenditure, since the voucher covered only about half the cost of private school. On balance, winners' gross school fees exceeded those of losers by about 70% of the amount they received from the voucher. This increase comes from the discrete jump in fees for those who switched from public to private schools, and from the fact that winners who would have gone to private school anyway spent more on school fees, possibly because they went to better schools. Moreover, lottery winners worked less, so that, on balance, households winning the lottery actually devoted more resources to education than the voucher face value. We also estimate that the voucher program cost the government about $24 more per winner than the cost of creating a public school placement. These costs to participants and the government are likely to have been outweighed by the benefits of the voucher to participants -- in the form of the economic return to increased educational attainment and test scores.

The paper is organized as follows. Section I provides background on education in Colombia and describes the PACES program in more detail. Section II discusses data and presents descriptive statistics from our survey. Section III discusses the effect of the program on school choice and basic educational outcomes. Section IV reports the effect of winning a voucher on test scores and non-education outcomes. Section V discusses the use of lottery win/loss status as an instrument to identify the causal effect of receiving a scholarship. Finally, Section VI looks at the effect of the program on household and government expenditure, and compares program costs with the benefits to participants.

I. Background

The Colombian government established the PACES program in late 1991 as part of a wider decentralization effort and in an attempt to expand private provision of public services (King *et al*, 1997). The program, which was partly funded by the World Bank, was also motivated as an effort to quickly expand school capacity and to raise secondary school enrollment rates (King, Orazem, and Wolgemuth, 1998).[2] Although 89% of Colombia's primary-school age children were enrolled in 1993, only 75% of the eligible population was enrolled in secondary schools. Among children of eligible age in the poorest quintile of the population, 78% were enrolled in primary school, but only 55% were enrolled in secondary school (Sanchez and Mendez, 1995; note that secondary school covers grades 6-11 in Colombia.)

The PACES program targeted low-income families by offering vouchers only to children residing in neighborhoods classified as falling into the two lowest socioeconomic strata (out of 6 possible strata). Applicants had to submit a utility bill to establish residential location and voucher eligibility. A second important feature of program targeting was that vouchers were offered only to children attending public primary schools. This restriction further excluded upper-income households since almost half of children from the richest income quintile attended private primary schools. Studies by Morales-Cobo (1993) and Ribero and Tenjo (1997) suggests that the targeting was largely effective in Bogota.

PACES vouchers were worth only about US$190 at the time of our survey. The maximum voucher value was set initially to correspond to the average tuition of low-to-middle cost private schools in Colombia's three largest cities. Schools charging less than the vouchers' face value received only their usual tuition. PACES vouchers became less generous over time because they did not keep up with inflation, and hence recipients had to supplement vouchers with additional payments to cover school fees. Our survey data show

---

[2]PACES was launched in November 1991 with advertisements in print and on radio soliciting applicants in participating cities (Calderon, 1996). A World Bank (1993) report on Colombian secondary schools notes that most schools operated 2 or 3 shifts and that some towns have little room for additional pupils in spite of projected enrollment growth. Other problems mentioned in the report include poor primary-school preparation, weak school management, lack of teacher preparedness, lack of textbooks, and shortages of other supplemental materials. The early 1990s was a general period of reform and liberalization in Colombia; see, for example, Kugler (1999).

BEST COPY AVAILABLE

matriculation and monthly fees for private schools attended by voucher applicants in 1998 averaged about $340. By way of comparison, the average annual per-pupil public expenditure in Colombia's public secondary school system in 1995 was just over $350 (DNP, 1999), and public school parents in our sample typically had to pay tuition or fees of roughly $58. Per capita GNP in Colombia is now around $2,280 (World Bank, 1999).

To qualify for a voucher, applicants must have been entering the Colombian secondary school cycle which begins with grade 6, and be aged 15 or under. They must also have been admitted to a participating secondary school (i.e., one that would accept the voucher).[3] Participating schools had to be located in participating towns, which included all of Colombia's largest cities. Just under half of private schools in the 10 largest cities accepted vouchers in 1993.

Participating schools tended to serve lower-income pupils, and to have lower tuition than non-participating private schools. Schools with a vocational curriculum were also over-represented among those in the program. Initially, vouchers were good at both for-profit and non-profit schools, but after 1996, for-profit schools were excluded.[4] The number of vouchers in use in any one year peaked at roughly 90,000 in 1994 and 1995. There were approximately 3.1 million secondary school pupils in Colombia in 1995, 37% of whom attended private schools. In Bogota, roughly 58% of 567,000 secondary school pupils attended private school.

Test score comparisons reported by King *et al* (1997) show achievement levels in participating private schools were very close to those in public schools, though significantly below achievement levels in non-participating private schools. Similarly, pupil-teacher ratios and facilities were similar in public and participating private schools, while non-participating private schools had lower pupil-teacher ratios and the

---

[3] Background information in this section is taken from King *et al* (1997), Calderon (1996), and unpublished ICETEX documents.

[4] Problems were reported with low-quality schools created to exploit the vouchers. But Calderon (1996) notes that even before the non-profit restriction was imposed, only 15% of Bogota's voucher students attended such institutions.

best facilities. Clearly, then, relatively elite private schools opted-out of the PACES program. One reason for this may have been a reluctance to interact with the bureaucracy in the Colombian Institute for Education, Credit and Training Abroad (ICETEX), a government agency that ran the program. Moreover, vouchers were insufficient to cover much of the tuition at more expensive schools, and some school managers probably viewed the prospect of an influx of pupils from low-income backgrounds as undesirable. On the other hand, many private schools in Colombia serving low-income populations apparently welcomed the PACES program.

Voucher recipients were eligible for automatic renewal through eleventh grade, when Colombian high school ends, provided the recipient's academic performance warranted promotion to the next grade. Students failing a grade were supposed to be dropped from the PACES program. Figures from Calderon (1996) show that, on average, 77% of recipients renewed their vouchers, and estimates from our data are similar. By way of comparison, the national high school promotion rate was about 70%. Students who transferred from one participating private school to another could, in principle, transfer the voucher to the new school. In practice, however, our survey suggests many students who transferred schools after winning lost their vouchers.

Cities and towns used lotteries to allocate vouchers when demand exceeded supply. Municipal governments paid 20% of the voucher cost, while the central government paid 80%. Each municipality decided how many vouchers to fund, subject to a maximum allocated to towns by the central government. This allocation was determined by estimating the shortfall between primary school enrollment and the available space in public secondary schools. Voucher award rates therefore varied considerably by city and year, depending on the ratio of applicants to available vouchers. Regional ICETEX offices worked with individual municipalities to determine the number of vouchers to be funded, to check school requirements for participation, and to monitor implementation of the program. The Bogota ICETEX office provided software and instructions to regional offices for the purposes of random selection of applicants in cases of over-subscription. We obtained copies of lists of lottery winners and losers from ICETEX offices.[5]

---

[5]In a few cities, the local ICETEX office assigned vouchers based on pupils' primary school performance instead of randomly.

II. The Applicant Survey

A. Descriptive Statistics

Beginning in the summer of 1998, we interviewed roughly 1,600 PACES applicants, stratifying to obtain approximately equal numbers of winners and losers. Interviewing was limited to the 1995 and 1997 applicant cohorts from Bogota and the 1993 applicant cohort from Jamundi, a suburb of Cali. These years and cities were chosen for a combination of scientific and practical reasons. The largest and longest-running voucher program was in Bogota, and our survey team is based there. Cali is Colombia's second largest city and therefore also important, but almost no Cali applicants had phones, so we concentrated on a suburb, Jamundi. Telephones were used for the majority of interviews, primarily to reduce costs, but also because of interviewer safety and logistical considerations. In principle, the lottery was random *within* localities and conditional on whether households have access to a telephone (even if it is a neighbor's phone). The results should therefore yield internally valid estimates of the causal effect of the program on voucher applicants with access to a telephone in surveyed cities. Over 80% of applicants had access to a phone, and in the Bogota 1995 cohort, 88% had access to a phone.

Table 1 reports descriptive statistics for the sampling frame, attempted contacts and completed interviews.[6] There were 6,156 applicants in the three applicant cohorts of interest. We attempted to interview almost 3,000 applicants, obtaining an overall response rate of 54% and a response rate of almost 61% for the 1997 Bogota lottery. The higher response rate in the most recent lottery is not surprising since contact information for 1997 applicants is more recent. Interviews were completed with 55% of lottery winners and 53% of lottery losers. Although this response rate is far from ideal, the fact that winners and losers were almost equally likely to be interviewed is encouraging because the question of sample selection bias turns on whether voucher status is correlated with response probabilities (see, e.g., Angrist, 1997). Because response probabilities are virtually uncorrelated with voucher status, there should be little bias from our failure to

---

[6]The data appendix provides additional information about the survey.

interview all applicants.[7]

The typical applicant was about 13 years old at the time of application, while average age on the survey date varied from 13 for 1997 applicants to 17 for 1993 applicants. About half of the applicants were male. Roughly 85% of applicants were still in school, enrolled in grades ranging from 6th for the 1997 cohort to 8th or 9th for the 1993 cohort. The descriptive statistics also show that almost 90% of the applicants we interviewed started 6th grade in private school. This reflects the fact that eligibility for PACES vouchers was conditional on admission to a participating private school. Thus, most lottery losers went to private school anyway, at least for one year. On the other hand, only 63% of applicants were still in private school as of the survey date.

B. Personal Characteristics and Voucher Status

There is little evidence of any association between win/loss status and the individual characteristics measured in our data from Bogota, although winners and losers are less comparable in the 1993 Jamundi cohort. This can be seen in Table 2, which reports means and differences by win/loss status for all applicants in the study population, for sampled applicants, and for the sample of completed surveys. The sampling process began with lists showing applicant ID numbers, names, addresses, and phone numbers, separately for winners and losers. To obtain demographic characteristics for all applicants, whether surveyed or not, we imputed applicant age using ID numbers (which incorporate birthdays). We note, however, that our strategy of imputing ages is subject to error since 13% of applicants have invalid ID numbers.[8] We excluded

---

[7] The vast majority of non-responders were people we could not reach by telephone, either because they had moved or because the telephone number we had no longer worked. Roughly 3% of families contacted refused to answer. The only significant difference in response rates by win/loss status is for the Jamundi cohort. In what follows, we present results for the Bogota 1995 cohort and the combined cohorts separately. Complete follow-up is the holy grail of education research. Even careful evaluation studies using randomized and quasi-randomized designs (e.g., Rouse, 1998; Krueger and Whitmore, 2001) are based on samples with substantial loss to follow-up. Similarly, Howell *et al* (2000) report follow-up rates similar to ours for US voucher trials in three cities.

[8] Plausible IDs have 11 digits, the first 6 of which record birth dates. Correct ID numbers satisfy an equation involving a control digit and the other 10 digits. Random deviations, say, because of incomplete recording, transposition of digits, or errors in a single digit, fail to satisfy this algorithm with very high probability. The check formula is as follows. Let the jth digit be d_j, for j=1,10 and d_{11} be the control digit. Let $\{c_j; j=1,11\}$ be the sequence

observations in which the applicant was younger than 9 or older than 25. In practice, this restriction affected few observations. A second variable from the applicant record is gender. We used first names to assign sex for about 80% of the applicants. A final variable from the applicant record is a dummy for whether the record included a phone number.

Winners and losers have similar telephone access, age, and sex mix in the 1995 and 1997 Bogota data. As a further check on randomness, we compared win rates by school in those schools with more than 20 applicants to city averages in the Bogota data from 1995. No school had a win rate that differed significantly from the city average. In the Jamundi-93 sample, however, there are significant differences in average age and gender by win/loss status. Because the differences between winners and losers in the Jamundi lottery may indicate non-random assignment of vouchers, and because the 1997 Bogota cohort is too recent for a good reading on some outcomes, we present results from the Bogota-95 sample separately from the results for the pooled sample including all three cohorts.

III. Impact on Scholarship Use, School Choice, and Schooling

Our estimates of lottery effects are based on the following regression model:

$$y_{ic} = X_i'\beta_0 + \alpha_0 Z_i + \delta_c + \varepsilon_{ic}, \qquad (1)$$

where y_{ic} is the dependent variable for child i from application cohort c (defined by city and year); X_i represents a vector of individual and survey characteristics like age, sex, and whether the survey was telephone or in-person; Z_i is an indicator for whether child i won the voucher lottery; and δ_c is a cohort effect. The coefficient of interest is α_0. We estimate (1) using three sets of control variables: "no controls", i.e., excluding the X_i variables; "basic controls" including the X_i variables; and "basic plus barrio controls" which includes the X_i variables plus 19 neighborhood dummies in the Bogota-95 sample.

{5,4,3,2,7,6,5,4,3,2,1}. Then MOD{[$\sum_j c_j d_j$]/11}=0 establishes the ID number is valid.

A. Effects on Scholarship Use and School Choice

We begin with a simple analysis of the effect of winning the lottery on private school scholarship receipt and the choice between public and private school. The most immediate effect of the lottery was to increase the likelihood of receiving a private school scholarship. This can be seen in the first row of Table 3, which shows that at the time of our survey, voucher winners were 51 percentage points more likely than losers to have been using some kind of scholarship. Not all winners were using their PACES vouchers in the survey year. This is because 15% of winners were not in school at all, and another 16% were in public schools, and therefore ineligible for scholarships. Some lottery winners also lost their voucher after repeating a grade (7%), while 5% switched to non-participating private schools or failed to complete the paperwork for a transfer. Others attended schools that stopped accepting vouchers or lost their vouchers for unreported reasons. Just as not all winners were using a scholarship, some losers obtained scholarships from programs other than PACES and one loser was awarded a PACES voucher after re-applying the following year.

At the time of the survey, enrollment rates were .83 for losers and .85 for winners in the Bogota-95 sample, an insignificant difference. The estimates in Table 3 also show that most PACES applicants entered 6th grade in a private secondary school, and most finished 6th grade whether or not they won a voucher. But lottery winners were 6-7 percentage points more likely than losers to have begun 6th grade in private school, and 15-16 percentage points more likely to be in private school at the time of our survey. The effect of winning the PACES lottery on the probability of private school attendance was even larger in 7th grade, probably because losers were more likely to have left private school by then.

These results suggest the decision between public and private school was sensitive to variation in the price of private school induced by the program, while the decision whether to attend school was not.[4] This is consistent with a model in which those households most willing and able to pay for education attend private school; a middle group attends public school; and those least willing or able to pay do not attend at all. In this

[4]We can convert the private-school enrollment effects to an elasticity as follows. PACES vouchers reduced the marginal cost of private school attendance by about 50%, while vouchers increased private school enrollment in 7th grade by about 17%. The implied elasticity of private enrollment with respect to marginal cost is therefore .34.

case, no one is on the private school/no school margin, and so small subsidies to private education do not directly increase overall enrollment.[10] Since many public schools in Colombia were turning away applicants due to overcrowding, PACES is likely to have opened up places in public school for other pupils by reducing public-school queuing.

B. Effects on Schooling

Lottery winners completed more schooling than losers, and were less likely to repeat grades. For example, lottery losers had completed 7.5 years of schooling at the time of our survey, but winners in the 1995 Bogota sample completed an additional .12-.16 years (.8 years in the full sample). As noted earlier, there was no statistically significant effect on enrollment. The effect on years of schooling and the lack of an effect on enrollment is primarily the result of a reduced probability of grade repetition for winners. This is reflected in a sharp increase in the likelihood lottery winners had finished 8th grade as of the survey date, with a smaller impact on 7th grade completion. In the Bogota-95 sample, over 20% of losers had repeated a grade since beginning 6th grade, and almost 20% repeated 6th grade. But the probability of grade repetition was reduced by 5-6 percentage points for lottery winners.

The estimates of α_0 change little as the list of control variables changes, a result to be expected since the voucher lottery was random. The estimation results are also similar in the Bogota-95 and full samples, and are largely invariant to the inclusion of neighborhood effects. Estimates and standard errors for the Bogota 1995 sample also change little in models with school effects.

Separate results by sex, reported in Table 4, show moderately larger effects on educational attainment for girls, though the pattern of sex differences in the effects on private school enrollment are not clear cut. Results for the Bogota-95 sample show male lottery winners with an insignificant 0.12 more years of schooling while female lottery winners obtained 0.14 years more of schooling, a statistically significant effect.

---

[10]PACES subsidies were initially large enough to cover the entire cost of private school, and may have shifted recipients from no school to private school when the program started. However, the voucher value was later eroded by inflation.

Differences by sex are more pronounced in the full sample, with an insignificant 0.06 more years of schooling for boys, and a statistically significant 0.12 more years of schooling for girls. It should also be noted that while effects for boys are almost entirely due to grade repetition, the effects for girls appear to come from both reduced grade repetition and additional time spent in school.[11]

The greater probability of 8th grade completion and lower repetition rates for lottery winners seem like desirable outcomes. In fact, high rates of grade repetition in Latin America are widely seen as symptomatic of poorly functioning public schools.[12] But the interpretation of these effects is complicated by the fact that pupils who failed a grade were supposed to forfeit PACES vouchers. Private schools may therefore have had an incentive to promote pupils with vouchers even if their performance did not meet normal promotional standards. To explore this possibility, we look at effects on test scores and non-educational outcomes in the next section.

IV. Effects on Test Scores and Non-Education Outcomes

A. Effects on Test Scores

We tested children from the 1995 applicant cohort in three Bogota neighborhoods. These neighborhoods were chosen because they had relatively large numbers of winners and losers, and because of the availability of suitable testing sites. The tests were administered in 1999, approximately one year after our household survey. The test sample was drawn from applicants for whom we had survey data. Participants were solicited by telephone, followed by hand delivery of letters describing the purpose of the test and inviting pupils to be tested. Those who failed to appear on the test day were invited again for a second testing, except at the last sitting. To encourage participation, refreshments were provided at each site, and each test concluded with the raffle of a bicycle and other prizes. Pupils were also given 5 or 10 thousand pesos (US

---

[11] There is little evidence that the effect of winning the voucher varied with applicants' socioeconomic strata of residence or parents' education. However, estimates for subgroups are imprecise.

[12] For example, Psacharopolous and Velez (1993) and Harbison and Hanushek (1992) use repetition rates as a measure of school quality in Colombia and Brazil.

$3.23 or $6.45) to cover travel costs. The invitation letter noted the offer of refreshments, travel reimbursement, and raffle. See the data appendix for additional details on the testing set-up.

Our evaluation used *La Prueba de Realizacion*, a grade-specific multiple-choice achievement test for native Spanish speakers, published by Riverside. We administered only the mathematics, reading, and writing subtests, each taking about 30 minutes. This test was chosen because Colombian educators participated in test development and the test had been used previously in Colombia (Cole *et al*, 1993). An appendix table compares test results from the Hispanic-American test-norming populations for grades 9 and 10 with the results from our test. Colombian 9th graders scored lower than American pupils in mathematics, but they had reading skills slightly better than American 10th graders. The average Colombian writing score was close to the average score for American 10th graders.

The Test Sample

The pupils invited to be tested were not a random sample, but they have characteristics similar to those in the full Bogota 1995 sample. This can be seen in the first two columns of Table 5, which describe the characteristics of all 1,176 Bogota 1995 applicants surveyed, and the 473 applicants invited for testing. Of those invited, 283 were tested, a response rate of 60%. Those tested were younger, and came from a poorer background as measured by father's education and wages, than the no-shows. Importantly, however, there is no significant relationship between the probability of being tested and voucher status. Also encouraging is the fact that, conditional on taking the test, there is little evidence of differences in personal characteristics between winners and losers. Differences by voucher status are reported in column 5 of Table 5.

Test results

Table 6 reports estimates of the effect of winning the voucher lottery on test scores. Columns 1 and

2 of Table 6 show results from models with and without covariates.[13] Columns 3 and 4 present the results of estimating a single voucher coefficient for stacked subject results, in models with a pupil random effect. That is, we estimated:

$$y_{is} = X_i'\beta_0 + \alpha_0 Z_i + \delta_i + \varepsilon_{is}, \quad (2)$$

where y_{is} is pupil i's score in subject s, and δ_i is a random effect used to adjust standard errors for the fact that there is likely to be within-pupil correlation across subjects. Note that test score results are reported in standard deviation units.

Lottery winners scored just over .2 standard deviations more than lottery losers, though this difference is (not surprisingly, given the small test sample) only marginally significant. According to US norms for *La Prueba*, 2/10 of a standard deviation is roughly the score gain associated with one additional school year (Cole *et al*, 1993). This effect should probably therefore be seen as large, in view of the fact that subjects were tested three years after applying to the program. Lottery winners also scored higher on all subtests, though the only significant difference is for reading scores (t=1.8). The results for the stacked subjects, reported in columns 3 and 4, also show marginally significantly higher scores for lottery winners, with the largest effects for models that stack math and reading scores only.

Models estimated separately for boys and girls generate larger and more precise effects for girls than boys. For example, the estimated effect on total points for girls, reported in column 2 of Panel B for models with covariates, is .26 (s.e.=.12). The corresponding estimate for boys, reported in Panel C, is .17 (s.e.=.18). The finding of a stronger effect on girls echoes some of the survey results.

Earlier we noted that the fact that lottery winners were less likely to repeat grades may have been due to a reduction in promotion standards for lottery winners, as well as any increased learning or change in school quality. Further evidence that the grade repetition results are not due solely to schools' lowering the bar for promotion of winners is provided by comparing the test scores of winners and losers who were promoted.

---

[13]The results in column 1 and 3 are from models that include site dummies only. The results in columns 2 and 4 are from models that include controls for age, sex, parents' schooling, strata of residence, type of interview, and survey form.

To see this, suppose that both test scores and grade promotion depend on underlying learning, but the program itself had no effect on achievement. This scenario can be shown to imply that if promotion standards are relaxed for winners, then average test scores for lottery winners who are promoted should be lower than average test scores for lottery losers who are promoted.[14] In practice, the composite test scores of winners who were promoted are about .14 standard deviations *greater* than the scores of promoted losers, although the difference is not significant.

B. Other Outcomes

Table 7 reports estimates of the effect of winning the lottery on non-educational outcomes. Approximately 1.6% of lottery losers from Bogota were married or living with a companion, a low proportion consistent with the fact that the average age of survey respondents was about 15. Since this outcome is rare, we estimated probit models as well as linear probability models.

Both probit marginal effects and OLS estimates suggest that marriage and cohabitation were reduced for lottery winners, a marginally significant effect. There is also some evidence from the pooled sample that lottery winners were less likely to be working than losers, with the largest effects in Bogota. There is also a significant difference in hours worked. In particular, lottery winners worked 1.2 fewer hours per week than losers. This effect is larger and more precisely estimated for girls. The reduction in work may be due to income effects for the household, the greater time demands of private school relative to public school, or increased incentives for lottery winners to spend time studying so as to avoid failing a grade and losing the PACES voucher.

---

[14]Suppose schools promote if a random variable x is greater than a cutoff c, which takes on two values, c_W for winners and c_L for losers. Suppose $c_W < c_L$, but test scores, T, and the variable x are unaffected by winning the lottery. Then the expected test score for lottery losers who are promoted is $E(T| x > c_L)$. The expected score for winners will be a weighted average of this and $E(T| c_W < x < c_L)$. Average scores for promoted losers will therefore exceed average scores for promoted winners as long as $E(T|x)$ is increasing in x.

V. Instrumental Variables Estimates of Scholarship Effects

The analysis so far focuses on reduced form effects of winning the lottery. In the discussion of Table 3, however, we noted that some lottery losers were awarded other scholarships, while some winners failed to use or retain their PACES scholarships. This section reports 2SLS estimates of the effect of ever receiving any scholarship using voucher win/loss status as an instrumental variable (IV). While only 6% of lottery losers used a scholarship at the time of the survey, 24% had used a scholarship at some point. In contrast, 90% of winners used a scholarship at some time. The 2SLS estimates based on this difference are necessarily larger than the reduced form effects of winning the lottery since winning the lottery is only imperfectly correlated with receiving a scholarship.

The assumption that a *scholarship use* dummy satisfies an exclusion restriction in an instrumental variables (IV) setup motivates 2SLS estimation of the equation:

$$y_i = X_i'\beta_1 + \alpha_1 s_i + \xi_i, \tag{3}$$

where s_i is a dummy for scholarship use, and X_i is the vector of "basic controls" used in previous tables. The associated first-stage relationship using Z_i as an instrument is

$$s_i = X_i'\gamma + \pi Z_i + \eta_i, \tag{4}$$

The estimate of π is about .66 (s.e.=.021), so the second-stage estimates can be expected to be about 50% larger than the corresponding reduced form estimates. The interpretation of α_1 in this case is as an approximate effect of treatment on the subset of scholarship users who would not have used a scholarship without PACES (Imbens and Angrist, 1994).

The 2SLS estimate of the effect of scholarship use on highest grade completed is about .2 in the Bogota-95 sample and .13 in the full sample. These estimates are reported in Table 8. 2SLS estimates of voucher effects on the probability of finishing eighth grade are 13-15 percentage points, nearly a 25% increase in completion rates. This is large by the standards of the literature on the enrollment effects of financial aid for college (see, e.g., Fuller, Manski, and Wise, 1982; Kane, 1995), though we know of no comparable results for subsidies to private secondary schools. Another interesting result is the 2SLS estimate of the effect on

test scores, .29, somewhat smaller than the corresponding OLS estimate. The 2SLS estimates are likely to be more useful for predicting the impact of scholarship programs on new scholarship recipients than are the reduced form effects, which are diluted by take-up rates less than one and the availability of alternative financing.

VI. Impact on Household and Government Expenditure

This section discusses the impact of the program on household and government budgets. We begin by showing that approximately 70% of voucher funds flowed to education, with the remainder going to educational spending that households would have made without the voucher. Taking into account the reduction in work by lottery winners suggests that winning the lottery induced households to devote more net resources to education. The higher fees paid by voucher winners are due primarily to winners' greater likelihood of attending private school. However, there is also some evidence that applicants who would have attended private schools anyway traded up to more expensive private schools in response to winning the voucher. Since the voucher did not reduce the cost of private school at the margin, this result weighs against the simplest models of education as human capital investment without credit constraints. The results in section B suggest that it cost the government about $24 more per lottery winner to provide school places through PACES than through the public system. Finally, section C aggregates the impact on households, schools, and the government budget, arguing that the total social costs of providing additional school places through the PACES voucher system were small, and therefore dwarfed by the benefits of the program to participants. The analysis in this section uses data for the 1995 Bogota applicant cohort only.

A. Impact on Household Educational Expenditure

Three years after the 1995 lottery in Bogota, about 55% of winners and 5% of losers were still receiving scholarships (a result from Table 3 repeated in the first row of Table 9 for the sample of observations with usable fee data). As noted earlier, 53% of losers were still in private school in the survey

year, with an increase of about 15% for winners.

Among applicants to the Bogota 95 lottery, winners received an average of $74 more in scholarship aid than losers, result reported in the fourth row of Table 9. Conditional on receiving a scholarship, scholarship amounts were similar for winners and losers, at roughly $200. The estimates in Table 9 also show gross matriculation and tuition fees were $52 greater for lottery winners than losers. Thus, the 1998 voucher expenditures of $74 per winner caused an increase of $52 in gross fees for winners, about 70% of the extra amount received by winners on average. The remaining $22 of voucher funds presumably increased non-educational expenditures by lottery winners.[15]

Aside from paying for school fees, households bear the opportunity cost of the effort students devote to education. The estimates for non-educational outcomes in Table 7 suggest lottery winners spent 1.2 fewer hours working each week. According to our survey data, the average hourly wage was 71 cents.[16] Assuming that students work 48 weeks per year, this reduction corresponds to an opportunity cost of 1.2 x $.71 x 48 weeks, approximately $41. Combining the increase of $52 in expenditures on fees and the $41 of lost earnings, we estimate that PACES lottery winners devoted $93 more to education than losers in the survey year, or 126% of the $74 in extra scholarship assistance they received.

Disaggregating Effects on Fees

While winning households spent about $52 more on school fees, on average, this average conceals important heterogeneity. Since vouchers covered only part of the cost of private school, families with children who were induced to switch to private school increased their educational expenditure sharply. However, most of the applicants who lost the lottery started private school in 6th grade anyway, and over half were still in

---

[15]The estimated displacement of private expenditure is even lower when a more comprehensive expenditure measure is used. Lottery winners report an estimated $84 more in comprehensive scholarship assistance (i.e., including expenditure on uniforms and textbooks) and an extra $74 more in comprehensive educational expenditure.

[16]Conditional on working, the average daily wage in our sample is $5.71. We estimate the hourly wage assuming a 7-hour work day.

private school in the survey year. So most vouchers were received by applicants who would have attended private school without the vouchers.

Simple models of education as human capital investment with perfect credit markets suggest that since PACES vouchers were worth only $190 per year, while most private schools cost over $300 per year, vouchers were infra-marginal. In other words, vouchers were not large enough to have caused households to increase educational spending by choosing a more expensive private school.[17] On the other hand, winning the voucher could have led households to choose more expensive private schools if educational spending is limited by credit constraints or if education has consumption value as well as investment value.

In a non-causal, purely accounting sense, the $52 of increased expenditure by winners on school fees can be decomposed into the effects of increased private school enrollment, and a switch to more expensive private schools by winners. Let Z be a dummy for lottery win/loss status as before, except we now drop i subscripts to simplify notation. Also, let R denote type of school attended (1 for private, 0 for public) and let F denote education expenditure. Gross school fees conditional on lottery win/loss status (i.e, fees paid by pupils without subtracting voucher amounts) are equal to

$$E[F|\,Z] = E[F|\,Z, R=1]P[R=1|\,Z] + E[F|\,Z, R=0]P[R=0|\,Z].$$

The overall change in fees is a linear combination of changes in public/private enrollment and changes in fees charged by school type. We can simplify the fee contrast between winners and losers using the fact that public school fees changed little and overall school enrollment was also affected little, so that $P[R=1|\,Z=1]-P[R=1|\,Z=0] \approx -\{P[R=0|\,Z=1]-P[R=0|\,Z=0]\}$. Then we have the accounting relationship:

$$\begin{aligned} &E[F|\,Z=1]-E[F|\,Z=0] \\ &= \{E[F|\,Z=1, R=1]-E[F|\,Z=1, R=0]\}\{P[R=1|\,Z=1]-P[R=1|\,Z=0]\} \\ &\qquad + P[R=1|\,Z=0]\{E[F|\,Z=1, R=1]-E[F|\,Z=0, R=1]\}. \end{aligned} \tag{5}$$

In words, the overall fee increase is caused by the private-public fee difference for winners, times private-

[17]To see this, note that if education is pure human capital investment, people choose schools so that a school costing one dollar more generates exactly one more dollar of present discounted earnings. For people who would in any case have attended a school costing more than $190, the voucher does not affect this first order condition.

school enrollment effects of the program, plus the win/loss contrast in fees for private-school pupils.[18] The right hand side components of (5) are as follows:

$$P[R=1|\, Z=1]-P[R=1|\, Z=0] = .15$$

$$E[F|\, Z=1, R=1]-E[F|\, Z=1, R=0] = 343-58=285$$

$$E[F|\, Z=1, R=1]-E[F|\, Z=0, R=1] = 11$$

$$P[R=1|\, Z=0] = .53.$$

This implies a total effect of $49, which is less than $52 because of the approximation used to simplify (5), with $43 due to school switching. Thus, in an accounting sense, the bulk of the change in household expenditure can be attributed to increased private school enrollment. However, for reasons discussed below, this decomposition provides an incomplete picture of the causal effect of the program on the fee distribution.

Causal effects on fees for families who would have sent their children to private school anyway are difficult to measure since we do not know who these families are. Simply comparing fees by win/loss status conditional on private school attendance (the second term in (5), above) leads to a biased estimate that is almost certainly too low. To see this, let F_0 be the public or private fee a pupil would pay if he or she loses the lottery and let F_1 be the public or private fee he or she would pay if he or she wins, and let R_0 and R_1 denote private school attendance if a pupil loses or wins the lottery respectively. Similarly, let f_0 be the private school fee a pupil would pay if he or she loses the lottery and let f_1 be the private school fee a pupil would pay if he or she wins. Thus $f_0 = F_0 \times R_0$ and $f_1 = F_1 \times R_1$. We imagine that these variables are defined for every pupil, though in practice, we can only observe F_0, R_0, and f_0 for losers and F_1, R_1, and f_1 for winners. $E[f_1 - f_0|\, f_0>0]$ is the effect on fees for those who would attend private school even if they were to lose the lottery. The observed contrast in fees for private school pupils can be written as follows

$$E[F|\, Z=1, R=1]-E[F|\, Z=0, R=1] = E[f_1 - f_0|\, f_0>0] + \{E[f_1|\, f_1>0] - E[f_1|\, f_0>0]\} \qquad (6)$$

---

[18]Without simplification the comparison is

$$E[F|\, Z=1]-E[F|\, Z=0]$$
$$= P[R=1|\, Z=0]\{E[F|\, Z=1, R=1]-E[F|\, Z=0, R=1]\} + E[F|\, Z=1, R=1]\{P[R=1|\, Z=1]-P[R=1|\, Z=0]\}$$
$$+ P[R=0|\, Z=0]\{E[F|\, Z=1, R=0]-E[F|\, Z=0, R=0]\} + E[F|\, Z=1, R=0]\{P[R=0|\, Z=1]-P[R=0|\, Z=0]\}.$$

The term in curly brackets comes from selection bias in the conditional-on-positive contrast. Under mild assumptions, this term is negative.[19] Assuming, as seems likely, that $f_1 \geq f_0$, the left hand side therefore is a lower bound on the causal effect, $E[f_1 - f_0 | f_0>0]$.

The parameter $E[f_1 - f_0 | f_0>0]$ is not identified without further assumptions (see, e.g., Chamberlain, 1986), though the previous discussion suggests we can treat the observed contrast in fees for private school students as a lower bound. Under mild assumptions, we can obtain a reasonably tight upper bound on this and a related family of parameters: $E[f_1 - f_0 | f_0>m_0(\theta)]$ where $m_0(\theta)$ is the θ-quantile of the losers' fee distribution. By choosing $m_0(\theta)=0$, we bound $E[f_1 - f_0 | f_0>0]$, while picking points at higher quantiles, we measure the effect on those who would have spent more on private schooling in the absence of the lottery. For example, we can bound the effect of winning the lottery on private school fees for those who would have spent more than the voucher amount ($190) on private school fees in the absence of the lottery. This result is stated formally below:

Proposition. Suppose that $F_1 \geq F_0$. Let $m_0(\theta)$ be the θ quantile of the distribution F for losers, with $m_1(\theta)$ defined similarly for winners. Then

$$E[f_1-f_0 | f_0>m_0(\theta)] \leq \{E[F | Z=1, F>m_1(\theta)] - E[F | Z=0, F>m_0(\theta)]\}/P[R=1 | Z=0, F>m_0(\theta)]. \quad (6)$$

Proof. Define $F_M = F_0 1(F_0 \leq m_0(\theta)) + F_0 1(F_0 >m_0(\theta), R_0=0) + F_1 1(F_0 >m_0(\theta), R_0=1)$. Note that $F_M = F_1$ for households above the quantile who would have attended private schools anyway. Otherwise, $F_M = F_0$. So $F_1 \geq F_M \geq F_0$. Let $p_\theta = P[R_0=1 | F_0>m_0(\theta)]$. Then

---

[19]This is easy to show in a model where $f_1=h(f_0)$ for any increasing transformation. More generally, winners who attend private school only if they win probably attend cheaper private schools than those who attend regardless in a variety of plausible models. Suppose, for example, that school quality complements ability and higher quality schools are more expensive. Then low-ability children attend public schools if they lose the lottery and attend cheap private schools if they win the lottery, while high-ability children attend expensive private schools whether or not they win a voucher. Alternatively, consider a model with credit constraints in which the poor attend public school; the rich attend private school; and the very rich attend expensive private schools. Then lottery winners who would have attended public school if they lost the lottery will attend cheaper schools than lottery winners who would have attended private school even if they lost.

$$E[F_M \mid F_0 > m_0(\theta)] - E[F_0 \mid F_0 > m_0(\theta)]$$

$$= \{p_\theta E[F_1 \mid F_0 > m_0(\theta), R_0=1] + (1 - p_\theta)E[F_0 \mid F_0 > m_0(\theta), R_0=0]\} - E[F_0 \mid F_0 > m_0(\theta)].$$

Note that

$$E[F_0 \mid F_0 > m_0(\theta)] = p_\theta E[F_0 \mid F_0 > m_0(\theta), R_0=1] + (1 - p_\theta)E[F_0 \mid F_0 > m_0(\theta), R_0=0].$$

Therefore,

$$E[F_M \mid F_0 > m_0(\theta)] - E[F_0 \mid F_0 > m_0(\theta)] = p_\theta E[F_1 - F_0 \mid F_0 > m_0(\theta), R_0=1].$$

Since $f_0=R_0F_0$, this implies

$$\{E[F_M \mid F_0 > m_0(\theta)] - E[F_0 \mid F_0 > m_0(\theta)\} / p_\theta = E[f_1-f_0 \mid f_0 > m_0(\theta)],$$

which is the quantity we seek to bound. Also, since $F_1 \geq F_M \geq F_0$ for all applicants, $E[F_1 \mid F_1 > m_1(\theta)] \geq E[F_1 \mid F_0 > m_0(\theta)] \geq E[F_M \mid F_0 > m_0(\theta)]$, and we have

$$E[F_1 \mid F_1 > m_1(\theta)] - E[F_0 \mid F_0 > m_0(\theta)] \geq E[F_M \mid F_0 > m_0(\theta)] - E[F_0 \mid F_0 > m_0(\theta)].$$

By randomization, $E[F_1 \mid F_1 > m_1(\theta)] = E[F \mid Z=1, F>m_1(\theta)]$ and $E[F_0 \mid F_0 > m_0(\theta)] = E[F \mid Z=0, F>m_0(\theta)]$ and $p_\theta = P[R=1 \mid Z=0, F>m_0(\theta)]$, which implies (6) and completes the proof.

To see why the upper bound works in the case where $\theta=0$, note that $E(F_M-F_0)$ is the average difference between winners' and losers' fees due to households who would have attended private schools in any case trading up to more expensive private schools. This is less than the observed difference in total fee payments by win/loss status, $E(F_1-F_0)$. The econometric intuition for this result is that in parametric sample selection models, controlling for the probability of sample selection eliminates selection bias. Comparing winners and losers at the same quantiles equalizes the "probability of selection" if $F_1 = h(F_0)$ for some monotone increasing transformation, h(). In fact, with no public school fees, the bound is exact when $F_1 = h(F_0)$. More generally, dividing by $P[R=1 \mid Z=0, F>m_0(\theta)]$ corrects for the fact that some of those with positive fees were attending public school, and the bound applies even without a deterministic link between F_0 and F_1.

Estimates of (6) are reported in Table 10, along with a lower bound using the biased comparison for quantiles analogous to (6), i.e., $E[F \mid Z=1,R=1]F>m_0(\theta)]-E[F \mid Z=0,R=1]F>m_0(\theta)]$. As noted earlier, this is a plausible lower bound because of negative selection bias. Note also that any reasonable behavioral model would predict that a family that spent less than the voucher amount on private school without a voucher would spend more after the voucher. We therefore focus on bounding effects that are conditional on paying pre-

voucher fees equal to at least the voucher amount, roughly $190.

Among losers paying at least $190 in fees, the average fee was $371. Almost all of these pupils were in private school (in fact, some reports of public school fees above $190 are probably in error; others refer to a handful of elite public schools that charge significant fees). The lower bound on $E[f_1-f_0| f_0>190]$ is about $3, but the upper bound is $38. The voucher amount of $190 is the .52 quantile of the fee distribution. Above this amount, the bounds are tighter. The estimated upper bounds above .6 range from $31 to $48, while the lower bounds range from $9 to $28. The lower bound estimates are not significantly different from zero at the 5% level. In some cases, however, the lower bounds are close to a 10% significance level, while the upper bounds allow for effects on the order of 10% of fee costs and 20% of the voucher value. Thus, it seems likely that winners in the upper half of the fee distribution spent 5-10% more on private schools than they otherwise would have. This implies that the marginal propensity to spend voucher income on more expensive private schools was non-trivial, counter to a simple model of human capital investment without credit constraints.

Price discrimination by private schools

Another potential source of increased expenditure on fees by winners is price discrimination. There is little evidence that private schools discriminated by charging more to applicants with PACES scholarships. The easiest way for schools to price discriminate was to offer scholarships to those less likely to be able to afford education at the full price, i.e., applicants without PACES vouchers. In practice, however, we found little evidence of price discrimination. Our survey indicates that roughly 6% of losers in private school received a school scholarship, while 3% of winners in private school received a school scholarship. This is a small and insignificant difference. Per lottery winner, i.e., without conditioning on attending private school, the difference is only about 1%.

B. Impact on the Government Budget

The PACES program was established in part to expand secondary school enrollment without using the public system. We estimate that the program increased public educational expenditure by about $24 per lottery winner, relative to the cost of accommodating these pupils in public school. As discussed in Section III, winners were no more likely than losers to attend school, but the program probably did expand overall school enrollment by freeing up places in public schools as lottery winners transferred to private schools.

To see where the $24 figure comes from, note that the probability of attending public school, reported in Table 9, fell by 0.14 for lottery winners. The average per-pupil cost of a secondary school slot was about $350, excluding implicit rental for school facilities. In the short run, the marginal cost of public school slots may differ from the average cost, but in the long run, it seems reasonable to assume marginal and average costs will be similar. Assuming the marginal cost of providing public school places equals the average cost, adding school spaces through PACES reduced long-run expenditure on public schools by 14% of $350 or roughly $50 per winner, so the extra public educational expenditure per lottery winner is about $74 (to pupils) - $50 (in reduced public school costs) = $24. Moreover, allowing the marginal cost to differ from average cost by $100 either way still leads to voucher program costs in the 10-40 dollar range.

C. Overall Cost and Benefits

The extra society-wide educational resource cost per lottery winner differs from the roughly $24 of extra public education expenditure, since households used part of the voucher funds to offset education costs they would have incurred privately, and households lost income from their children's work. The average lottery winner received $74 more than the average loser in scholarship assistance, but spent only $52 more on gross school fees. Lottery winners earned $41 less than losers through work. Winning households' net resource contribution was therefore $52 (additional school fees) + $41 (reduced earnings) - $74 (voucher) = $19. This implies that the society-wide additional educational resource cost per lottery winner was approximately $24 (government) + $19 (households) = $43.

The comparison of costs and benefits should take account of the fact that three years of costs were incurred prior to our survey. The total cost of the program can therefore be estimated by multiplying the annual resource cost times the roughly 3 years winners received vouchers, for a total of about 3×24 = $72 in additional public educational expenditure and 3×$43=$129 in total societal resource cost. Actual costs are probably somewhat higher, however, since voucher take-up rates declined over time, with 88% of winners having ever used a voucher, and only 49% using it in the survey year. Multiplying costs by 88%/49% for the first and second years yields an upper bound on the three-year cost of the program of about $195 using the $43/year figure for social costs.

These costs are very likely small relative to the benefits for participants. Although lottery winners gave up current earnings, they completed an additional .12-.16 grades and scored approximately .2 standard deviations higher on tests. Among US Hispanic students who took the same test, the difference in test scores between 7th and 8th graders, or between 8th and 9th graders, was also about .2 standard deviations, so the achievement gain from winning the lottery may be as large as that associated with a full year of schooling. Our estimates using a recent Colombian labor force survey show returns to schooling of about 10%. If the gain from the program is only the economic return to an additional 0.12 years of schooling, the program raised winners' wages by 1.2% per year, whereas if it is equal to that from a full year of schooling it raised wages by 10%. Annual earnings of parents in our sample were about $2,400 per worker, and PACES applicants should be able to earn more, since the average parent had only 5.9 years of education while the average applicant had already completed 7.5 years and was still in school at the time of our survey. We therefore assume the expected earnings of applicants are $3000. Thus, PACES seems very likely to raise lottery winners' wages by $36 per year, and might raise wages by as much as $300 per year if higher test scores have a grade-equivalent payoff. Discounted over applicants working lives, these benefits easily outweigh the social costs of the voucher program, which are probably no more than $195.

A more complete cost-benefit analysis would take into account the program's effects on non-participants. Pupils left behind in public schools may have been hurt by the departure of motivated classmates

for private schools, or alternatively, public schools may have responded positively to increased competition, a possibility considered by Hoxby (2000) and Bettinger (2001b). Such general equilibrium effects cannot be assessed by comparing lottery winners and losers. But since the partial equilibrium cost-benefit analysis is clear-cut, and since only 15% of winners moved from public to private schools, any negative external effects on non-participants would have to have been extraordinarily large to outweigh program benefits.

VII. Summary and Conclusions

Colombia's PACES program provides an unusual opportunity to assess the effect of demand-side education financing in a Latin American country where private schools educate a substantial fraction of pupils. The PACES program is of special interest because many vouchers were assigned by lottery, so program effects can be reliably assessed. Our results suggest that lottery winners benefitted from higher educational attainment, primarily as a consequence of reduced grade repetition, as well as from higher test scores and a lower probability of teen cohabitation or employment. Most of the results suggest PACES vouchers had a stronger effect on the education of girls than on the education of boys.

PACES vouchers affected outcomes through a number of channels. First, lottery winners were more likely to have attended participating private schools, and these schools may be better than public schools. Moreover, because voucher recipients who failed a grade risked losing vouchers, lottery winners had an incentive to devote more effort to school. Finally, vouchers may have allowed some households who would have chosen private schools in any case to attend more expensive private schools. The net effect is such that the benefit of voucher awards were more than enough to offset the costs. In work in progress, we are assessing longer term consequences of voucher receipt, and hope to be able to document effects on secondary school completion rates. An implication of our findings to date is that demand-side programs like PACES can be a cost-effective way to increase educational attainment and academic achievement, at least in countries like Colombia with a weak public school infrastructure and a well-developed private education sector.

DATA APPENDIX

1. Surveys

The sampling frame for the surveys were the lists of PACES winners and losers obtained from ICETEX offices. We selected applicants at random from these lists. Those with phones were interviewed first. Applicants were called up to 9 times total and 3 times/day to obtain an interview, with numbers of contacts similar for winners and losers. In general, we interviewed parents or guardians and not the applicants themselves. Some interviewing was conducted in person; household interviews are distinguished by the inclusion of a dummy in the estimation. We developed a pilot survey that was used to test questions and interviewing strategies. The pilot survey ultimately differed little from the main survey so pilot responses are included in the data set analyzed here and indicated by a survey-form dummy included in the analyses.

Interviewers were college students attending Javierna University De Colombia, in Bogota. The interviewers were selected and trained by one of us (Bloom). Responsibility for interviewing later passed to two of the most experienced interviewers. Surveys were checked and coded by the interviewers, but interviewers did not code their own interviews. Responses were also checked by a research assistant in Boston. We tried to blind interviewers to the win/loss status of respondents for as long as possible, and had each interviewer collect data from applicants in both groups.

2. Testing

There were four test dates. On the first, we tested applicants from Suba; on the second, we tested Suba no-shows; on the third date, we tested applicants from La Candelaria and downtown areas nearby. Finally, on the last date we tested third-date no-shows, plus applicants from the Ciudad Bolivar neighborhood and surrounding areas.

Sample sizes and response rates were as follows. 473 applicants were invited for testing, 283 (60%) were tested. Response rates at individual test sites were as follows: 77 Suba applicants were invited, 50 (65%) were tested on dates 1 or 2, one subject did not do the math test; 109 Candalaria applicants were

invited, 83 (76%) were tested on dates 3 or 4; 287 Ciudad Bolivar applicants were invited and one showed up uninvited, 150 (52%) were tested on date 4. Response rates were higher at the first two sites because no-shows had more than one opportunity to be tested. Test score means and pupil characteristics were similar across sites. Site-specific treatment effects were not significantly different from zero.

Scores are reported in the tables as the standardized (mean zero, standard deviation 1) number right. The standardization uses means and standard deviations in sample of test takers. The appendix table presents a comparison of our test scores with those for the *La Prueba* norming population.

REFERENCES

Angrist, Joshua D. "Conditional Independence in Sample Selection Models." *Economics Letters*, February 1997, 54(2), 103-112.

Behrman, Jere; Sengupta, Piyali and Todd, Petra. "Progressing through PROGRESA: An Impact Assessment of Mexico's School Subsidy Experiment." Draft, 2000.

Bellow, Rosemary and King, Elizabeth M. "Educating Women: Lessons from Experience." in King and Hill (eds.), *Women's Education in Developing Countries: Barriers, Benefits, and Policy*, 1993, Baltimore: Johns Hopkins Press.

Bettinger, Eric. "Do Private School Vouchers Affect Test Scores and Why? Evidence from a Private School Scholarship Program." Case Western Reserve, Department of Economics, January 2001, mimeo.

Bettinger, Eric. "The Effect of Charter Schools on Charter Students and Public Schools." Case Western Reserve, Department of Economics, March 2001, mimeo.

Calderon, Alberto. "Voucher Programs for Secondary Schools: The Colombian Experience." Human Capital Development and Operations Policy Working Paper, Washington, DC: The World Bank, 1996, (http://www.worldbank.org/education/economicsed/finance/demand/related/wp_00066.html).

Chamberlain, Gary. "Asymptotic Efficiency in Semi-Parametric Models with Censoring." *Journal of Econometrics*, July 1986, 32(2), 189-218.

Cole, Nancy S.; Trent, E. Roger and Wadell, Dena C. *La Prueba Riverside de Realización en Español: Teacher's Guide and Technical Summary*. Chicago: The Riverside Publishing Company, 1993.

Cox, Donald and Jimenez, Emmanuel. "The Effectiveness of Private and Public Schools: Evidence from Two Developing Countries." *Journal of Development Economics*, November 1990, 34(9), 99-121.

DNP, *Sistema de Indicadores Sociodemograficos para Colombia (SISD) 1980-1997* Boletin No. 21, p. 58, Bogota: Departmento Nacional de Planeacion, June 1999.

Evans, William N. and Schwab, Robert M. "Finishing High School and Starting College: Do Catholic Schools Make a Difference?" *Quarterly Journal of Economics*, November 1995, 110(4), 941-974.

Fuller, Winship C.; Manski, Charles F. and Wise, David A. "New Evidence on the Economic Determinants of Postsecondary Schooling Choices." *The Journal of Human Resources*, Fall 1982, 17(4), 477-498.

Glewwe, Paul; Kremer, Michael and Moulin, Sylvie, "Textbooks and Test Scores: Evidence from a Prospective Evaluation in Kenya." Harvard University, mimeo, September 2000.

Green, Jay; Peterson, Paul and Du, Jiangtao. "The Effectiveness of School Choice in Milwaukee: A Secondary Analysis of Data from the Program's Evaluation." Harvard Program in Education Policy and Governance, August 1996, Occasional Paper 96-3.

Harbison, R.W. and Hanushek, Eric A. *Educational Performance of the Poor: Lessons from Rural Northeast Brazil*. New York: Oxford University Press, 1992.

Hoxby, Caroline M. "Does Competition Among Public Schools Benefit Students and Taxpayers." *American Economic Review*, December 2000, 90(5), 1209-1238.

Howell, William G.; Wolf, Patrick J.; Peterson, Paul E. and Campbell, David E. "Test-Score Effects of School Vouchers in Dayton, New York, and Washington: Evidence from Randomized Field Trials." Paper presented at the annual meeting of the American Political Science Association, Washington, D.C., September 2000.

Imbens, Guido W. and Angrist, Joshua D. "Identification and Estimation of Local Average Treatment Effects." *Econometrica* , March 1994, 62(2), 467-476.

Jacoby, Hanan. "Borrowing Constraints and Progress Through School: Evidence from Peru." *Review of Economics and Statistics*, February 1994, 76(1), 151-160.

James, Estelle. "Why Do Different Countries Choose a Different Public-Private Mix of Educational Services." *Journal of Human Resources*, Summer 1993, 28(3), 571-592.

Jimenez, Emmanuel; Lockheed, Marlaine E. and Paqueo, Vicente. "The Relative Efficiency of Private and Public Schools in Developing Countries." *The World Bank Research Observer*, July 1991, 6(2), 205-218.

Kane, Thomas. "Rising Public College Tuition and College Entry: How Well Do Public Subsidies Promote Access to College?" NBER Working Paper No. 5164, 1995.

King, Elizabeth; Orazem, Peter and Wolgemuth, Darin. "Central Mandates and Local Incentives: The Colombia Education Voucher Program." Working Paper No. 6, Series on Impact Evaluation of Education Reforms, Development Economics Research Group, The World Bank, February 1998.

King, Elizabeth; Rawlings, Laura; Gutierrez, Marybell; Pardo, Carlos and Torres, Carlos. "Colombia's Targeted Education Voucher Program: Features, Coverage and Participation." Working Paper No. 3, Series on Impact Evaluation of Education Reforms, Development Economics Research Group, The World Bank, September, 1997.

Krueger, Alan and Whitmore, Diane. "The Effect of Attending a Small Class in the Early Grades on College-Test Taking and Middle School Test Results: Evidence from Project STAR." *Economic Journal*, January 2001, 111(468), 1-28.

Kugler, Adriana D. "The Impact of Firing Costs on Turnover and Unemployment: Evidence from the Colombian Labour Market Reform." *International Tax and Public Finance*, August 1999, 6(3), 389-410.

McEwan, Patrick and Carnoy, Martin. "Privatization through Vouchers in Developing Countries: The Cases of Chile and Colombia." Teacher's College mimeo, 1999.

Morales-Cobo, Patricia. "Demand Subsidies: A Case Study of the PACES Voucher Program." Universidad de los Andes, Economics Department, Bogota, 1993, Processed.

Myers, David; Peterson, Paul; Mayer, Daniel; Chou, Julia and Howell, William. "School Choice in New York City After Two Years: An Evaluation of the School Choice Scholarships Program." Program on Education Policy and Governance, mimeo, August 2000.

Myers, David, Peterson, Paul, and Howell, William. "An Evaluation of the New York City: School Choice Scholarships Program: The First Year." Harvard's PEPG Occasional Paper Series, 2000.

Neal, Derek, "The Effects of Catholic Secondary Schooling on Educational Achievement." *Journal Labor Economics*, Part 1 January 1997, 15(1), 98-123.

Patrinos, Harry A. and Ariasingham, David L. *Decentralization of Education: Demand-Side Financing*. Washington, DC: The World Bank, 1997.

Psacharopolous, George; Tan, J., and Jimenez, E. *Financing Education in Developing Countries: An Exploration of Policy Options*. Washington DC: The World Bank, 1986.

Psacharopolous, George, and Velez, Eduardo. "Educational Quality and Labor Market Outcomes: Evidence from Bogota, Colombia." *Sociology of Education*, April 1993, 66(3), 130-145.

Rauch, James E. and Evans, Peter B. "Bureaucratic Structure and Bureaucratic Performance in Less Developed Countries." *Journal of Public Economics*, January 2000, 75(1), 49-71.

Ribero, Rocío and Tenjo, Jaime. University de los Andes, Department of Economics Working Paper, 1997.

Rouse, Cecilia Elena. "Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program." *Quarterly Journal of Economics*, May 1998, 13(2), 553-602.

Sanchez, Fabio and Mendez, Jairo. "Por Que los Niños Pobres No Van A La Escuela? (Determinantes de la asistencia escolar en Colombia)." Mimeo, Departmento Nacional de Planeacion Republica de Colombia, 1995.

The PROBE Team, *Public Report on Basic Education in India*. Oxford: Oxford University Press, 1999.

US Department of Education, Office of Education Research and Improvement. *The Condition of Education 1998*, NCES 98-013, Washington, DC: USGPO, 1998.

The World Bank, *World Development Report 1998/99*. New York: Oxford University Press, 1999.

The World Bank, Research Department, *Staff Appraisal Report: Colombia, Secondary Education Project*, Human Resources Operations Division, Latin America and the Caribbean Region, Report No. 11834-CO, November 19, 1993.

Table 1. Sample Design and Survey Response Data

Variable	Bogota 95	Bogota 97	Jamundi 93[1]	Combined Sample
A. Population				
N	4044	1770	342	6156
% Awarded Vouchers	58.8	84.7	50.0	65.8
B. Attempted Interviews				
N	2249	457	279	2985
% Awarded Vouchers	50.0	51.6	50.2	50.3
Response Rate	.523	.606	.591	.542
Winner Rate	.528	.619	.650	.553
Loser Rate	.518	.593	.532	.531
C. Completed Interviews				
N	1176	277	165	1618
% Awarded Vouchers	50.4	52.7	55.2	51.3
Household Visit	.054	.004	.782	.120
Age at time of Application[2]	12.6	12.4	12.5	12.6
	(1.3)	(1.4)	(1.9)	(1.4)
Age on Survey Date	15.0	13.1	16.9	14.9
(from Survey Data)	(1.3)	(1.4)	(1.5)	(1.7)
Male	.510	.495	.424	.499
Started 6th in Private	.910	.880	.669	.880
Started 7th in Private	.763	.731	.626	.744
Currently in Private School	.618	.738	.506	.628
Highest Grade Completed	7.6	6.0	8.6	7.4
	(.940)	(.480)	(1.1)	(1.1)
Currently In School	.836	.957	.778	.851

Standard deviations for non-binary variables are shown in parentheses. Sample sizes may differ across rows. Data are from 1998 household surveys.

[1]Jamundi is near Cali.

[2]"Age at time of Application" is imputed from the National Identification number reported on the application.

Table 2. Personal Characteristics and Voucher Status

Dependent Variable	Bogota 95		Bogota 97		Jamundi 93		Combined Sample	
	Loser's Mean	Coeff on Winning	Loser's Mean	Coeff on Winning	Loser's Mean	Coeff on Winning	Loser's Mean	Coeff on Winning
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Data from PACES Application								
Has Phone	.882	.009	.828	.029	.301	.068	.825	.017*
		(.011)		(.025)		(.052)		(.010)
Age at time of	12.7	-.086*	12.7	-.227**	12.7	-.383**	12.7	-.133**
Application	(1.3)	(.045)	(1.5)	(.102)	(1.5)	(.162)	(1.4)	(.040)
Male	.493	.013	.484	.007	.386	.114**	.483	.019
		(.017)		(.044)		(.055)		(.015)
N	1519	3661	256	1736	166	334	1941	5731
B. Data for All Attempted Contacts								
Has Phone	1	--	1	--	.370	.082	.938	.008
						(.059)		(.006)
Age at time of	12.8	-.118**	12.6	-.193	12.8	-.595**	12.7	-.177**
Application	(1.3)	(.060)	(1.5)	(.136)	(1.6)	(.183)	(1.4)	(.052)
Male	.500	-.007	.488	-.020	.372	.102*	.486	.001
		(.022)		(.048)		(.061)		(.019)
N	1035	2067	212	448	135	272	1382	2787
C. Survey Data								
Age at time of	15.0	-.013	13.2	-.259	17.2	-.375*	14.9	-.107
Survey	(1.4)	(.078)	(1.4)	(.171)	(1.4)	(.217)	(1.7)	(.068)
Male	.501	.004	.527	-.047	.365	.110	.492	.008
		(.029)		(.061)		(.077)		(.025)
Mother's Highest	5.9	-.079	5.9	.654*	4.4	1.46**	5.8	.183
Grade Completed	(2.7)	(.166)	(2.7)	(.371)	(2.7)	(.494)	(2.7)	(.144)
Father's Highest	5.9	-.431**	5.5	.929**	5.2	.737	5.8	-.042
Grade Completed	(2.9)	(.199)	(2.5)	(.388)	(2.9)	(.640)	(2.9)	(.170)
Mother's Age	40.7	-.027	38.7	-.146	43.6	-.736	40.6	-.076
	(7.3)	(.426)	(6.6)	(.808)	(8.8)	(1.42)	(7.4)	(.362)
Father's Age	44.4	.567	41.9	.265	45.5	1.92	44.1	.537
	(8.1)	(.533)	(7.3)	(.973)	(9.1)	(1.61)	(8.1)	(.453)
Mother's Wage	.020	.012	0	.014	0	.014	.015	.012
(>2 Min Wage)		(.010)		(.010)		(.014)		(.008)
Father's Wage	.100	.005	.088	-.008	.133	-.092*	.101	-.003
(>2 Min Wage)		(.021)		(.043)		(.056)		(.018)
N	583	1176	131	277	74	165	788	1618

Notes: Standard deviations for non-binary variables appear in Columns 1, 3, 5, and 7. Robust standard errors are reported in columns 2, 4, 6, and 8. Regressions in Panels A and B include controls for city and year of application. Regressions in Panel C show results from models with additional controls for type of survey and instrument, neighborhood of residence, and month of interview. Sample size varies by row. The maximum sample size is included in each panel. The sample for the outcome "Age at time of Application" restricts the sample to applicants between 9 and 25 years old. This restriction excludes 2 individuals whose imputed age at application was greater than 62.

BEST COPY AVAILABLE

Table 3. Educational Outcomes and Voucher Status

Dependent Variable	Bogota 95				Combined Sample	
	Loser's Means	No Ctls	Basic Ctls	Basic +19 Barrio Ctls	Basic Ctls	Basic +19 Barrio Ctls
	(1)	(2)	(3)	(4)	(5)	(6)
Using any Scholarship in Survey Year	.057	.509**	.504**	.505**	.526**	.521**
	(.232)	(.023)	(.023)	(.023)	(.019)	(.019)
Ever Used a Scholarship	.243	.672**	.663**	.662**	.636**	.635**
	(.430)	(.021)	(.022)	(.022)	(.019)	(.019)
Started 6th in Private	.877	.063**	.057**	.058**	.066**	.067**
	(.328)	(.017)	(.017)	(.017)	(.016)	(.016)
Started 7th in Private	.673	.174**	.168**	.171**	.170**	.173**
	(.470)	(.025)	(.025)	(.024)	(.021)	(.021)
Currently In Private School	.539	.160**	.153**	.156**	.152**	.154**
	(.499)	(.028)	(.027)	(.027)	(.023)	(.023)
Highest Grade Completed	7.5	.164**	.130**	.120**	.085**	.078*
	(.960)	(.053)	(.051)	(.051)	(.041)	(.041)
Currently In School	.831	.019	.007	.007	-.002	-.002
	(.375)	(.022)	(.020)	(.020)	(.016)	(.016)
Finished 6th Grade	.943	.026**	.023*	.021*	.014	.012
	(.232)	(.012)	(.012)	(.011)	(.011)	(.010)
Finished 7th Grade (excludes Bog 97)	.847	.040**	.031	.029	.027	.025
	(.360)	(.020)	(.019)	(.019)	(.018)	(.018)
Finished 8th Grade (excludes Bog 97)	.632	.112**	.100**	.094**	.077**	.074**
	(.483)	(.027)	(.027)	(.027)	(.024)	(.024)
Repetitions of 6th Grade	.194	-.066**	-.059**	-.059**	-.049**	-.049**
	(.454)	(.024)	(.024)	(.024)	(.019)	(.019)
Ever Repeated after lottery	.224	-.060**	-.055**	-.051**	-.055**	-.053**
	(.417)	(.023)	(.023)	(.023)	(.019)	(.019)
Total Repetitions since lottery	.254	-.073**	-.067**	-.064**	-.058**	-.057**
	(.508)	(.028)	(.027)	(.027)	(.022)	(.022)
Years in School since lottery	3.7	.058	.034	.031	.015	.012
	(.951)	(.052)	(.050)	(.050)	(.044)	(.043)
Sample Size	562		1147		1577	

Notes: Samples used to estimate 7th and 8th grade completion effects exclude Bogota 1997. The sample size for these outcomes is 1304 in columns 6-7. The regression estimates are from models that include controls for city, year of application, whether applicant has phone, age, type of survey and instrument, strata of residence, and month of interview.

Table 4. Educational Outcomes and Voucher Status, By Gender

Dependent Variable	Coefficient on Voucher Status					
	Bogota 1995				Combined Sample	
	Male		Female		Male	Female
	Loser's Means (1)	Basic Ctls (2)	Loser's Means (3)	Basic Ctls (4)	Basic Ctls (5)	Basic Ctls (6)
Started 6th in Private	.857	.082**	.897	.027	.058**	.077**
	(.351)	(.025)	(.304)	(.021)	(.023)	(.021)
Started 7th in Private	.646	.187**	.699	.143**	.166**	.177**
	(.479)	(.035)	(.460)	(.033)	(.031)	(.029)
Currently in Private School	.543	.136**	.535	.171**	.124**	.182**
	(.499)	(.039)	(.500)	(.039)	(.033)	(.033)
Highest Grade Completed	7.4	.124	7.6	.140**	.056	.122**
	(.990)	(.076)	(.934)	(.065)	(.062)	(.052)
Currently in School	.843	-.020	.819	.035	-.026	.029
	(.365)	(.029)	(.386)	(.027)	(.024)	(.022)
Finished 6th	.932	.014	.954	.032**	.003	.027**
	(.252)	(.018)	(.210)	(.013)	(.017)	(.012)
Finished 7th	.825	.026	.869	.041	-.003	.022
	(.380)	(.029)	(.338)	(.025)	(.024)	(.020)
Finished 8th	.589	.095**	.674	.105**	.066**	.078**
	(.493)	(.039)	(.470)	(.036)	(.030)	(.027)
Repetitions of 6th grade	.229	-.087**	.160	-.036	-.070**	-.033
	(.506)	(.037)	(.395)	(.030)	(.031)	(.023)
Ever Repeated after lottery	.254	-.083**	.195	-.029	-.076**	-.035
	(.436)	(.034)	(.370)	(.031)	(.028)	(.025)
Total Repetitions since lottery	.296	-.101**	.213	-.031	-.079**	-.037
	(.550)	(.042)	(.459)	(.033)	(.035)	(.026)
Calendar Years in School since lottery	3.7	-.029	3.6	.091	-.041	.081
	(.962)	(.077)	(.941)	(.063)	(.067)	(.055)
Sample Size	280	575	282	572	779	798

Notes: Robust standard errors are shown in parentheses in columns 2, 4, 5, and 6. Standard deviations for non-binary variables are shown in parentheses in columns 1 and 3. The regression estimates are from models that include controls for city, year of application, whether applicant has phone, age, type of survey and instrument, strata of residence, and month of interview.

Table 5. Descriptive Statistics for Test Administration Sample

Variable	Bogota 95	Applicants Invited	Applicants Who Took Test	Diff By Whether Took Test	Diff By Voucher Status for Test Takers
	(1)	(2)	(3)	(4)	(5)
A. Demographics/Family Background					
Age on Survey Date	15.0	15.0	14.8	-.391**	-.156
	(.039)	(.061)	(.080)	(.124)	(.160)
Male	.510	.517	.529	.029	.001
	(.015)	(.023)	(.030)	(.047)	(.061)
Mother's Highest Grade Completed	5.9	5.7	5.7	-.046	-.196
	(.084)	(.128)	(.162)	(.263)	(.326)
Father's Highest Grade Completed	5.7	5.6	5.3	-.748**	-.026
	(.099)	(.147)	(.186)	(.300)	(.038)
Mother's Wage^2 (>2 Min Wage)	.026	.033	.029	-.010	.038*
	(.025)	(.008)	(.010)	(.017)	(.021)
Father's Wage^2 (>2 Min Wage)	.105	.122	.093	-.077**	.076*
	(.010)	(.017)	(.020)	(.036)	(.040)
B. Survey/Voucher Variables					
Proportion Invited to Exam	.402	--	--	--	--
	(.014)				
Proportion Attending Exam	.241	.598	--	--	--
	(.012)	(.023)			
Won Voucher	.504	.539	.556	.043	--
	(.015)	(.023)	(.030)	(.047)	
Currently Attending Private School	.618	.652	.698	.113**	.151**
	(.014)	(.022)	(.027)	(.045)	(.055)
Highest Grade Completed	7.6	7.6	7.7	.323**	.167
	(.027)	(043)	(.052)	(.086)	(.104)
Finished 8th Grade	.699	.706	.792	.213**	.099**
	(.013)	(.021)	(.024)	(.042)	(.048)
Number of Repetitions since lottery	.213	.220	.177	-.108**	-.099*
	(.014)	(.023)	(.029)	(.048)	(.057)
Sample Size (Panels A&B)	1176	473	283	473	283
C. Test Survey Data					
Applicant Age	--	--	15.9	--	.357
			(.135)		(.272)
Male	--	--	.486	--	.032
			(.030)		(.060)
Sample Size	--	--	283	--	283

Notes: Standard errors are reported in parentheses. Sample sizes may differ across rows. Column 4 shows the differences between students who took the exam and students who were invited to the exam and did not attend. Column 5 shows the difference between lottery winners and losers who took the exam.

Table 6. Test Results

	OLS Estimates (1)	OLS with Covariates (2)	RE Estimates (3)	RE with Covariates (4)
A. Test Scores for All Applicants				
Total Points	.217*	.205*		
	(.120)	(.108)		
Math Scores	.178	.153		
	(.118)	(.110)		
Reading Scores	.204*	.203*		
	(.122)	(.113)		
Writing Scores	.126	.128		
	(.121)	(.114)		
Pooled Test Scores			.170*	.148*
			(.095)	(.088)
Math and Reading Scores			.192*	.162*
			(.101)	(.096)
B. Test Scores for Female Applicants				
Total Points	.199	.263**		
	(.152)	(.120)		
Math Scores	.292**	.346**		
	(.142)	(.126)		
Reading Scores	.117	.152		
	(.156)	(.135)		
Math and Reading Scores			.204	.235**
			(.130)	(.117)
B. Test Scores for Male Applicants				
Total Points	.204	.170		
	(.183)	(.179)		
Math Scores	.010	.004		
	(.142)	(.031)		
Reading Scores	.276	.220		
	(.190)	(.176)		
Math and Reading Scores			.143	.087
			(.160)	(.160)

Robust standard errors are reported in parentheses. Test scores are in standard deviation units. Columns 2 & 4 include controls for applicant's age, gender, parents' schooling, strata of residence and type of survey and instrument. Columns 3 & 4 include random effects for each test subject. The row entitled "Pooled Test Scores" includes 3 observations for each student (one for each subject) while "Math and Reading Scores" includes only two observations per student.

Sample sizes are as follows:

N=283 for Reading and Writing Scores N=282 for Math and Total Points

N=846 for Pooled Scores N=568 for the Combined Math and Reading Scores

BEST COPY AVAILABLE

Table 7. Non-Educational Outcomes and Voucher Status

Dependent Variable	Coefficient on Voucher Status					
	Bogota 95			Combined Sample		
	Losers' Means	Basic Controls	Probit w/ Basic Controls	Losers' Means	Basic Controls	Probit w/ Basic Controls
	(1)	(2)	(3)	(4)	(5)	(6)
A. Male and Female						
Married or Living w/	.0160	-.0087	-.0066*	.0171	-.0094*	-.0065*
Companion	(.1256)	(.0059)	(.0038)	(.1297)	(.0056)	(.0034)
Has Child	.0338	-.0103	-.0079	.0303	-.0069	-.0055
	(.1809)	(.0096)	(.0075)	(.1714)	(.0079)	(.0062)
Applicant is	.1690	-.0297	-.0299	.1616	-.02651	-.0254*
Working	(.3751)	(.0205)	(.0184)	(.3684)	(.0171)	(.0153)
Number of Hours	4.881	-1.222*	--	4.417	-.8699	--
Working	(12.3)	(.6441)	--	(11.60)	(.5235)	--
Sample Size	562	1147	1147	760	1577	1577
B. Male						
Married or Living w/	.0036	-.0039	--	.0027	-.0027	--
Companion	(.0598)	(.0038)	--	(.0518)	(.0026)	--
Applicant is	.2321	-.0366	-.0336	.2252	-.0283	-.0294
Working	(.4230)	(.0331)	(.0324)	(.4183)	(.0278)	(.0261)
Number of Hours	6.421	-.6376	--	6.198	-.6231	--
Working	(13.69)	(1.072)	--	(13.31)	(.8859)	--
Sample Size	280	575	575	373	779	778
C. Female						
Married or Living w/	.0284	-.0100	-.0112	.0309	-.0113	-.0114*
Companion	(.1663)	(.0113)	(.0078)	(.1733)	(.0109)	(.0068)
Pregnant or Has	.0603	-.0151	-.0155	.0541	-.0092	-.0119
Child	(.2384)	(.0179)	(.0138)	(.2266)	(.0147)	(.0110)
Applicant is	.1064	-.0314	-.0262	.1005	-.0317	-.0240
Working	(.3089)	(.0235)	(.0189)	(.3011)	(.0196)	(.0156)
Number of Hours	3.351	-2.116**	--	2.704	-1.499**	--
Working	(10.57)	(.6527)	--	(9.36)	(.5240)	--
Sample Size	282	572	572	388	798	798

Notes: Results are for a sample with non-missing non-educational and educational outcomes. Columns 2 and 3 show results from models that include whether applicant had access to a phone, age, gender, type of survey and instrument, strata of residence, and month of interview. Columns 5 and 6 also include controls for city and year of application.

Table 8. OLS and 2SLS Estimates of the Effect of Ever Using a Private School Scholarship

Dependent Variable	Coefficient on Ever Used a Private School Scholarship				
		Bogota 95		Combined Sample	
	Loser's Mean	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Highest Grade Completed	7.5	.167**	.196**	.141**	.134**
	(.965)	(.053)	(.078)	(.042)	(.065)
In School	.831	.021	.010	.033*	-.003
	(.375)	(.021)	(.031)	(.017)	(.026)
Total Repetitions since lottery	.254	-.077**	-.100**	-.069**	-.091**
	(.508)	(.029)	(.042)	(.023)	(.035)
Finished 8th	.632	.114**	.151**	.108**	.127**
	(.483)	(.028)	(.041)	(.025)	(.038)
Test Scores – Total Points	-.099	.379**	.291*	--	--
	(1.0)	(.111)	(.153)		
Married or Living w/ Companion	.016	-.009	-.013	-.010*	-.014
	(.126)	(.006)	(.009)	(.006)	(.009)
N	562	1147		1577	

Notes: Results are from models that control for city, year of application, whether applicant had access to a phone, age, type of survey and instrument, strata of residence, and month of interview. The outcome "Finished 8th" in the combined sample does not include applicants to the Bogota 1997 voucher lottery. In the outcome "Test Scores – Total Points," the sample is restricted to those individuals who took the exam. In the 2sls specification, the endogenous regressor *used scholarship* is instrumented with a voucher win/loss dummy.

Table 9. Matriculation and Fee Effects for Bogota 1995 Applicants

Variable	Full Sample			Conditional on Private School Attendance		
	Loser's Means	No Ctls	Basic Ctls	Loser's Means	No Ctls	Basic Ctls
	(1)	(2)	(3)	(4)	(5)	(6)
Using any Scholarship in Survey Year	.054	.497**	.494**	.085	.709**	.696**
	(.227)	(.023)	(.023)	(.279)	(.027)	(.028)
Currently In Private School	.530	.156	.152	1	--	--
	(.500)	(.029)	(.028)			
Currently In Public School	.290	-.140**	-.146**	0	--	--
	(.454)	(.025)	(.025)			
Scholarship Value	16.0	74.3**	72.6**	29.8	101.8**	98.1**
	(64.2)	(5.4)	(5.4)	(85.8)	(7.6)	(7.9)
Scholarship Value (Conditional on >0)	199.0	-13.3	-11.3	211.1	-25.5	-22.0
	(122.4)	(19.1)	(18.5)	(118.2)	(19.1)	(18.5)
Gross School Fees	191.5	52.3**	48.1**	332.2	11.0	9.8
	(188.3)	(10.2)	(11.0)	(133.6)	(10.7)	(10.7)
Net School Fees	175.9	-22.0*	-24.5**	302.4	-90.7**	-88.2**
	(185.6)	(11.5)	(9.9)	(154.3)	(11.6)	(11.7)
Gross School Fees for Public Schools	54.6	3.2	1.0	--	--	--
	(109.4)	(13.4)	(13.8)			
Currently Using Scholarship *from the Private School*	.031	-.008	-.010	.059	-.026	-.031
	(.173)	(.011)	(.012)	(.235)	(.019)	(.020)
Sample Size	534	1085	1085	283	661	664

Notes: Standard deviations are reported in parentheses in columns 1 and 4. Robust standard errors are reported in parentheses in all other columns. Sample sizes differ from other tables because of missing fee data. The sample sizes differ slightly across rows because of missing data for "Currently Using Scholarship from the Private School" and the outcomes with the more comprehensive fee measure. The regression estimates in columns 3 and 6 are from models that include controls for whether applicant has phone, age, type of survey and instrument, strata of residence, and month of interview.

Table 10. Bounds on Voucher Effects on Fees, for Applicants Who Would Have Attended Private School Anyway

Dollar Cutoff	Corresponding Quantile of Losers' Fee Distribution (1)	Losers' Avg Fee Above Quantile (2)	Losers' Probability of Priv School Attendance (3)	Losers' Avg Priv School Fee Above Cutoff (4)	Upper Bound on Fee Effect (5)	Lower Bound On Fee Effect (6)
190.0	52nd Percentile	370.5	.973	368.1	38.4	2.9
		(104.7)	(.163)	(95.1)	(9.5)	(8.9)
283.2	60th Percentile	396.7	.981	392.5	31.4	9.2
		(94.4)	(.136)	(83.0)	(9.7)	(8.8)
335.8	70th Percentile	424.8	.981	419.3	32.1	17.7
		(92.7)	(.136)	(78.7)	(11.3)	(10.0)
370.1	80th Percentile	460.2	.972	452.8	36.5	14.0
		(96.0)	(.167)	(78.3)	(9.3)	(12.3)
427.0	90th Percentile	523.3	.943	511.9	48.3	27.7
		(101.0)	(.233)	(74.5)	(16.8)	(18.3)

Notes: Estimates are for Bogota 1995 applicants only. Bounds are conditional on fee payments exceeding the indicated dollar cutoffs. Standard deviations are reported in parentheses in columns 2-4. Standard errors are reported in parentheses in columns 5 and 6. The upper bound above a given percentile is computed in a two-step process. First, we estimate the difference between winners' and losers' matriculation fees conditional on being greater than the given quantile in their respective distributions. Second, we divide this difference by the probability that losers above the given percentile attend private school. The lower is computed by taking the differences between winners' and losers' fees conditional on private school attendance and conditional on being greater than the cutoff. See text and appendix for further details.

Appendix Table 1. Summary Statistics from *La Prueba de Realizacion*

Subject	US-Normed Population Grade 9	US-Normed Population Grade 10	Colombia PACES Results (~Grade 9)
Mathematics			
Mean	16.9	18.3	13.0
Std	5.0	5.0	4.9
Median	16.2	18.0	12.0
Reading			
Mean	18.8	20.8	21.4
Std	5.3	4.9	4.1
Median	19.0	21.2	22.0
Writing			
Mean	13.7	14.5	14.8
Std	3.8	3.5	3.3
Median	13.4	17.8	15.0

Notes:

The US-Norming population included 8851 students in grades K-12 from all states. Ninety-five percent of the students were from Arizona, California, Colorado, Illinois, New Mexico, or Texas. The Colombia-PACES results are based on 283 applicants to the 1995 Bogota lottery, tested between June and November of 1999.

To order any of these papers in hard copy, see instructions at the end of this list. To subscribe to all NBER Working Papers or the papers in a single area, see instructions inside the back cover.

Number	Author(s)	Title	Date
8296	Kala Krishna Cemile Yavas	Lumpy Consumer Durables, Market Power, and Endogenous Business Cycles	5/01
8297	Leora Friedberg	The Impact of Technological Change on Older Workers: Evidence from Data on Computer Use	5/01
8298	Bruce D. Meyer James X. Sullivan	The Effects of Welfare and Tax Reform: The Material Well-Being of Single Mothers in the 1980s and 1990s	5/01
8299	Robert E. Lipsey Fredrik Sjoholm	Foreign Direct Investment and Wages in Indonesian Manufacturing	5/01
8300	Thomas N. Hubbard	Affiliation, Integration, and Information: Ownership Incentives and Industry Structure	5/01
8301	Antoni Estevadeordal Alan M. Taylor	A Century of Missing Trade?	5/01
8302	Owen A. Lamont Richard H. Thaler	Can the Market Add and Subtract? Mispricing in Tech Stock Carve-Outs	5/01
8303	Giancarlo Corsetti Paolo Pesenti Nouriel Roubini	The Role of Large Players in Currency Crises	5/01
8304	Lisandro Abrego Raymond Riezman John Whalley	How Often are Propositions on the Effects of Customs Unions Theoretical Curiosa and When Should They Guide Policy?	5/01
8305	Michael M. Hutchison	A Cure Worse Than the Disease? Currency Crises and the Output Costs of IMF-Supported Stabilization Programs	5/01
8306	Brent Boning Casey Ichniowski Kathryn Shaw	Opportunity Counts: Teams and the Effectiveness of Production Incentives	5/01
8307	Ronald G. Ehrenberg Christopher L. Smith	The Sources and Uses of Annual Giving at Private Research Universities	5/01
8308	Konan Chan Louis K. C. Chan Narasimhan Jegadeesh Josef Lakonishok	Earnings Quality and Stock Returns	5/01

You can download these and other papers at the NBER Web site:

www.nber.org

Free searchable abstracts are also available at the site.

To order any of these papers in hard copy, see instructions at the end of this list. To subscribe to all NBER Working Papers or the papers in a single area, see instructions inside the back cover.

Number	Author(s)	Title	Date
8309	Tano Santos Pietro Veronesi	Labor Income and Predictable Stock Returns	5/01
8310	William J. Collins	The Labor Market Impact of State-Level Anti-Discrimination Laws, 1940-1960	5/01
8311	Andrew W. Lo Harry Mamaysky Jiang Wang	Asset Prices and Trading Volume Under Fixed Transactions Costs	5/01
8312	Guillermo Llorente Roni Michaely Gideon Saar Jiang Wang	Dynamic Volume-Return Relation of Individual Stocks	5/01
8313	Assaf Razin Chi-Wa Yuen	The "New Keynesian" Phillips Curve: Closed Economy vs. Open Economy	6/01
8314	David B. Gross Nicholas S. Souleles	Do Liquidity Constraints and Interest Rates Matter for Consumer Behavior? Evidence from Credit Card Data	6/01
8315	Michael P. Dooley Sujata Verma	Rescue Packages and Output Losses Following Crises	6/01
8316	Craig William Perry Harvey S. Rosen	The Self-Employed are Less Likely to Have Health Insurance Than Wage Earners. So What?	6/01
8317	Austan Goolsbee Amil Petrin	The Consumer Gains from Direct Broadcast Satellites and the Competition with Cable Television	6/01
8318	Angus Deaton	Health, Inequality and Economic Development	6/01
8319	Ted Joyce	Did Legalized Abortion Lower Crime?	6/01
8320	Douglas Staiger James H. Stock Mark W. Watson	Prices, Wages and the U.S. NAIRU in the 1990s	6/01
8321	David T. Ellwood	The Sputtering Labor Force of the 21st Century Can Social Policy Help?	6/01
8322	Robert Dekle Kenneth M. Kletzer	Domestic Bank Regulation and Financial Crises: Theory and Empirical Evidence from East Asia	6/01

You can download these and other papers at the NBER Web site:

www.nber.org

Free searchable abstracts are also available at the site.

To order any of these papers in hard copy, see instructions at the end of this list. To subscribe to all NBER Working Papers or the papers in a single area, see instructions inside the back cover.

Number	Author(s)	Title	Date
8323	Peter L. Rousseau Richard Sylla	Financial Systems, Economic Growth, and Globalization	6/01
8324	Rudi Dornbusch	Fewer Monies, Better Monies	6/01
8325	Rudi Dornbusch	Malaysia: Was it Different?	6/01
8326	Rudi Dornbusch	A Primer on Emerging Market Crises	6/01
8327	Andrew B. Bernard J. Bradford Jensen	Who Dies? International Trade, Market Structure, and Industrial Restructuring	6/01
8328	Dana Goldman Darius Lakdawalla	Understanding Health Disparities Across Education Groups	6/01
8329	Marianne Baxter	Social Security as a Financial Asset: Gender-Specific Risks and Returns	6/01
8330	Robert J. Barro	Economic Growth in East Asia Before and After the Financial Crisis	6/01
8331	John A. Tauras Patrick M. O'Malley Lloyd D. Johnston	Effects of Price and Access Laws on Teenage Smoking Initiation: A National Longitudinal Analysis	6/01
8332	John A. Tauras Frank J. Chaloupka	The Demand for Nicotine Replacement Therapies	6/01
8333	B. Douglas Bernheim Robert J. Lemke John Karl Scholz	Do Estate and Gift Taxes Affect the Timing of Private Transfers?	6/01
8334	James R. Markusen Keith E. Maskus	General-Equilibrium Approaches to the Multinational Firm: a Review of Theory and Evidence	6/01
8335	James R. Markusen Keith E. Maskus	A Unified Approach to Intra-Industry Trade and Direct Foreign Investment	6/01
8336	David H. Autor Mark G. Duggan	The Rise in Disability Recipiency and the Decline in Unemployment	6/01
8337	David H. Autor Frank Levy Richard J. Murnane	The Skill Content of Recent Technological Change: An Empirical Exploration	6/01

You can download these and other papers at the NBER Web site:

www.nber.org

Free searchable abstracts are also available at the site.

To order any of these papers in hard copy, see instructions at the end of this list. To subscribe to all NBER Working Papers or the papers in a single area, see instructions inside the back cover.

Number	Author(s)	Title	Date
8338	Enrique G. Mendoza	Credit, Prices, and Crashes: Business Cycles with a Sudden Stop	6/01
8339	Kevin H. O'Rourke	Globalization and Inequality: Historical Trends	6/01
8340	James M. Poterba	Taxation, Risk-Taking, and Household Portfolio Behavior	6/01
8341	Jagadeesh Gokhale Laurence J. Kotlikoff Todd Neumann	Does Participating in a 401(k) Raise Your Lifetime Taxes?	6/01
8342	Jeremy C. Stein	Agency, Information and Corporate Interest	6/01
8343	Joshua Angrist Eric Bettinger Erik Bloom Elizabeth King Michael Kremer	Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment	6/01

Copies of the above working papers can be obtained for $10.00 per copy (plus $10.00 per order for shipping for all locations outside the continental U.S.) to Working Papers, NBER, 1050 Massachusetts Avenue, Cambridge, MA 02138-5398. Pre-payment is required on all orders and may be made by check or credit card. Checks should be made payable to the NBER and must be in dollars drawn on a U.S. bank. If paying by credit card, include the cardholder's name, account number, and expiration date. For all orders, please be sure to include your return address and telephone number. Working papers may also be ordered by telephone (868-3900), fax (617-868-2742), or email (orders@nber.org).

You can download these and other papers at the NBER Web site:

www.nber.org

Free searchable abstracts are also available at the site.

NBER *Working Paper Subscription Order Form*

Please circle the price of the item(s) you wish to order.

	Domestic		Foreign (Air Mail)		Online Only	
	Standard	Academic	Standard	Academic	Standard	Academic
Full Subscription*	$1850	$1070	$2200	$1460	$1310	$530
Partial Subscriptions:						
Corporate Finance	245	115	300	145	185	55
Economic Fluctuations and Growth	345	195	430	270	245	95
Economics of the Elderly	110	75	125	85	70	35
Health and Health Care	195	125	250	160	130	60
Industrial Organization	110	75	125	85	70	35
International Finance and Macroeconomics	345	195	430	270	245	95
International Trade and Investment	345	195	430	270	245	95
Labor Studies	345	195	430	270	245	95
Monetary Economics	195	125	250	160	130	60
Productivity and Technological Change	130	90	145	110	85	45
Stocks, Bonds, and Foreign Currency	345	115	400	145	285	55
Taxation	345	195	430	270	245	95
Technical Working Papers	115	90	130	100	70	45
Historical Development of the American Economy	115	90	130	100	70	45

*A full subscription includes all topics listed under "partial subscriptions" except for Technical Working Papers and papers on the Historical Development of the American Economy. These must be ordered in addition to the full subscription. The full text of papers issued since November 1994 is available on-line. **On-line access is included in the price of domestic and foreign subscriptions.** If you prefer, you may purchase an "on-line only" subscription that would allow all users at your site electronic access to our working papers. Please see the pricing information listed above. New subscriptions begin the month following receipt of payment and run for twelve months.

PAYMENT OPTIONS: Please circle the price(s) of your selection(s) above.

By Check: (in U.S. dollars, issued by a U.S. bank)

NBER Subscriptions
National Bureau of Economic Research
1050 Massachusetts Avenue
Cambridge, MA 02138-5398 USA

By Phone: (617) 588-0315
By Fax: (617) 441-3895

With one of the following:

• VISA • MasterCard • American Express

Card Number: _____

Expiration Date: _____

Signature: _____

Please Print Name: _____

Address: _____

_____

_____

_____

Phone: _____

Fax: _____

E-mail: _____

For **on-line access,** please supply either of the following:

Domain Name: _____
*Your domain name is usually the information that comes after the @ in your e-mail address. For example, our domain name is "nber.org"

Network Number(s): _____
*Please see your Network Administrator to obtain your network number. See www.nber.org/reverse.html for more information.

BEST COPY AVAILABLE

The NBER Working Paper Series

Partial Subscription Options:

- Corporate Finance
- Stocks, Bonds, and Foreign Currency
- International Finance and Macroeconomics
- International Trade and Investment
- Monetary Economics
- Economic Fluctuations and Growth
- Productivity
- Taxation
- Labor Studies
- Economics of Health and Health Care
- Economics of the Elderly
- Industrial Organization
- Technical Papers
- Historical Papers

Fastest Access to Research Findings

NBER Working Papers provide the research findings of the leading academic research economists in the United States, often years before the same analysis is available in other professional publications. Papers are mailed as they are issued so that subscribers always have the latest information.

Subscription Options Tailored to Your Needs

Build your own personal library. Select only those NBER papers that suit your information needs by choosing a partial subscription to one or more of the topics.

Substantial Savings

All Subscriptions represent a substantial savings over the cost of purchasing single copies. We also offer special rates to faculty members and academic institutions.

Order Now for Prompt Delivery

For your convenience we accept credit card orders. See order form on opposite page for details.

You can download this and other papers at the NBER Web site:

www.nber.org

Free searchable abstracts are also available at the site.

NBER

National Bureau of Economic Research, Inc.
1050 Massachusetts Avenue
Cambridge, Massachusetts 02138

53